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EINSTEIN'S LUCKIEST THOUGHT*

We were taught at school that there is no place for reason in scientific discovery. Reason's place is in the "context of justification". But the context of discovery-exemplified by the wriggling, tail-biting snakelike devils which inspired Kekulé's discovery of the ring structure of benzene[1] – is one dominated by chance and "the Unconscious". Now, it is undeniable that thoughts – good or bad – come when they will, not when we want them to.[2] But the inquiring scientist who is inspired – by Lady Luck or whomever – with a novel idea must then and there decide to trash it or to keep it and continue to build upon it, at least for the time being. He cannot wait until the philosophers finally agree on a computer checkable logical syntax of inductive inference which, if applied to the relevant data, would constitute the appropriate context of justification. Indeed, the idea he has had can turn into a germ of discovery only by being woven into his ongoing critical, argumentative reflection about the scientific problem at hand. What whims of intellectual fancy are retained and further elaborated, and which forgotten, is determined, from day to day, and even from minute to minute, by promptly testing how they fit into this live tissue of thought. It is this, and this alone, that deserves the name of a "context of discovery". And, of course, it is eminently a place for reason. Indeed, there is no better way of ascertaining what we ought to understand by scientific reason than to look here, into a working scientist's reflections, and to identify that – whatever it is – which steers and coordinates them in the course of this research.

We are justifiably wary of "rational reconstructions" which try to fit such a context of discovery into some Procrustean idea of methodology and reason. But is there any hope of recovering past thought, if not *wie es eigentlich gewesen*, at least in a shape that we find persuasive? Well, hope there surely is, while we do not ourselves give it up. We have to muster, though, all our tact and good judgment, and bring them to bear on every scrap of documentary evidence: letters, stray notes, faulty early publications. Now, such has been the endeavor of the history of philosophy in its own field, since it was founded in 19th century Germany by Johann Eduard Erdmann, Eduard Zeller, Wilhelm Dilthey, etc.; and it is indeed no wonder that Alexandre Koyré, whom some of us regard as the father of modern history of science, should have begun his scholarly life as a historian of philosophy.

What makes the formative period of General Relativity especially attractive for the philosophical historian is, in the first place, Einstein's extraordinary intellectual clarity and his knack for essentials; but also the

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sheer length of his quest, which lasted almost nine years, so that the process of invention and discovery, insofar as it is visible, is shown in slow motion. During that period, Einstein published about a dozen and a half papers dealing with gravity – including two he wrote with Grossmann, and one he wrote with Fokker. He also wrote many letters, notebooks, etc. John Norton has demonstrated what treasures can be found among them.[3] Unfortunately, they are not yet generally accessible. So in the following remarks I take my cue from what is available in print.

Why did Einstein have to search for a new theory of gravity? In 1907 he wrote to Habicht that he was working on relativity theory in connection with the law of gravity, and that he thereby hoped to account for the so-far unexplained secular changes in the perihelion of Mercury.[4] Yet in 1913 he told the 85th *Naturforscherversammlung* in Vienna that Newton's Law of Gravity has proved to be "so exactly right that, from the standpoint of experience, there is no decisive ground for doubting its strict validity".[5] These clashing testimonies could be reconciled by recalling that in 1913 Einstein thought he had already found with his friend Grossmann the theory he was looking for – indeed, the main purpose of his address to the Naturforscher was to present it to them –, and he probably knew that it could not cope with Mercury's perihelion advance.[6] Thus – one might say – Einstein in his Vienna lecture chose to forget an anomaly which beset his own theory no less than Newton's, and described the latter as empirically impeccable, although that very anomaly had motivated his research on gravity in the first place.

But there is a less paltry way of dealing with this matter. In a much quoted passage of a letter written in 1952, Einstein told Max Born that

> even if no light deviation, no perihelion advance and no gravitational redshift were known, the gravitational equations [of General Relativity] would be convincing, because they avoid the inertial system – this phantom acting on everything, but upon which things do not react.

And he added:

> It is truly remarkable that men are mostly deaf to the strongest arguments, while they are always inclined to overestimate the precision of measurements.[7]

This passage was written in 1952, so it carries no authority for deciding what considerations guided Einstein's quest in 1907. It does, however, explain why Einstein could appear satisfied in 1913 with a theory which did not meet his earlier hopes. And it clearly suggests that when Einstein spoke to Habicht of a relativity theory of gravitation by which he hoped to solve the Mercury perihelion anomaly, the emphasis lay on 'relativity', not on 'Mercury'. A solution of the perihelion problem was, of course, to be hoped for, but rather as an extra bonus, something that would make the forthcoming theory more persuasive by showing that it was not merely an ad hoc way of smoothing out the discrepancy between the old celestial mechanics and the new electrodynamics of moving bodies.

That the latter was incompatible with Newton's theory of gravity had been duly noted by Poincaré in 'Sur la dynamique de l'électron'.[8] Relativistic electrodynamics required that "all forces, and in particular gravitation be affected by the Lorentz transformation in the same way as electromagnetic forces". Hence, Poincaré concluded, the force of gravity must depend, like the Lorentz force, not only on the relative positions, but also on the velocity of the interacting bodies; moreover, its effect on each body at a given time must be calculated on the basis of an earlier position and velocity of the other body, "as if gravitation had taken a certain time to propagate".

An attempt to reconcile gravity physics with Einstein's electrodynamics, based on the equivalence of inertial frames and the constancy of the speed of light, should have led to a theory like the one published by A. N. Whitehead in 1924. But if we turn to Einstein's earlier pronouncements on gravity, we find no indication that he ever sought for something along those lines. According to Einstein's 'Autobiographical Notes' of 1949 he did not waste time on such a project because he soon realized that it would compromise the equality between inertial and gravitational mass – or that, at any rate, it could not do justice to that equality "in a natural way". This persuaded him "that within the frame of the special theory of relativity there is no room for a satisfactory theory of gravitation".[9]

Now, in the same year 1907 in which Einstein, for the sake of a satisfactory account of gravity, began turning his back on his first theory of relativity, the most distinguished convert to the theory, Professor Max Planck in Berlin, in a paper entitled 'Zur Dynamik bewegter Systeme', cited the equality of inertial and gravitational mass among the "intuitions and principles commonly used as firmest buttresses for theoretical reflections of all sorts" which, in the light of the new physics, were being deprived of their purported universality.[10] For, he says,

> thermal radiation in a void space enclosed by reflecting walls surely possesses inertial mass; but does it also have gravitational mass? If – as it seems most likely – this question must be denied, the identity of inertial and gravitational mass, which is confirmed by all past experiments and is universally assumed, will obviously be destroyed.[11]

Why did Einstein, *circa* 1906, remain loyal to the received identity of inertial and gravitational mass, to the point of sacrificing to it his newborn brainchild? In the 'Autobiographical Notes' he recalls that "from very accurate experiments [. . .] it was empirically known with very high accuracy that the *gravitational* mass of a body is exactly equal to its *inert* mass."[12] But why was Einstein so willing to rely in this case on the precision of measurements, and even to extrapolate their results to every conceivable set of circumstances? The very clumsiness of his writing in the passage I have just quoted betrays that this was not just a matter of good scientific sportsmanship, of Einstein's resolve to play by the rules of inductive evidence. For, after all, he knew full well that a *very accurate* experiment can establish an equality *with very high accuracy* but never *exactly.*

To see better what is here at play, consider the Newtonian force exerted by a source of unit mass at the origin on a particle of mass *m* at point **r**. It is represented by either side of the equation:

$$(1)\qquad m(d^2\mathbf{r}/dt^2) = -\,mG\mathbf{r}/|\mathbf{r}|\mathbf{r}^2,$$

where G is the gravitational constant. The *m* on the left hand side stands for the particle's inertia or resistance to impressed forces, while the *m* on the right hand side denotes its (passive) gravitational charge, or susceptibility to the impressed force of gravity. Why should these two conceptually different physical quantities be the same? In Newton's natural philosophy a particle's inertia is given by its "quantity of matter", and second generation Newtonians were content to think that matter's universal susceptibility to gravitational attraction was also measured by its quantity.[13] But *quantitas materiæ*, a creature of scholastic theology,[14] could hardly pass for a ground of equality in 1907. So the equality of inertial and gravitational mass in Equation (1) appeared as a startling coincidence. The equality is, of course, confirmed by the identical downward acceleration experienced in our terrestrial environment by all bodies, regardless of their composition. But will their accelerations remain identical to one another, say, in a much stronger gravitational field? Might not inertia and gravitational charge be just very nearly equal in the limit of weak fields and low speeds? Why did not Einstein countenance this alternative in order to rescue special relativity? After all, he was never squeamish about issuing promissory notes which no data bank was ready to cash.

My last question assumes that Einstein at that time still viewed special relativity as something worth rescuing. But are we sure that he did? What if the theory already looked too unrelativistic for him to risk his credit on it? Later he will hold against special relativity is retention of "the inertial system", that is, its acknowledgement of a privileged family of reference frames. I believe that this consideration weighed heavily on his mind from the very start of his work on gravity. Indeed, I surmise that his unshakeable allegiance to the identity of gravitational and inertial mass stemmed from the promise which their identification held for the final suppression of "the inertial system". My surmise is backed by three paragraphs in the 'Autobiographical Notes' which come right after the last passage I quoted from that work. Allow me to paraphrase them:

(1) It then occurred to Einstein – "nun fiel mir ein" – that in a lab at rest in a practically uniform gravitational field of intensity **g** everything will appear the same as it would if no such field existed but the lab itself experienced a practically constant acceleration –**g** relative to the inertial frames.

(2) Thus, if one conceives the behavior of bodies with respect to a reference system of the latter sort as caused by a "real" (and not merely apparent) gravitational field, one will be entitled to regard this system as an "inertial system" with the same right with which any terrestrial lab is so regarded.

(3) Hence the existence of locally uniform gravitational fields which are, so to speak, at loggerheads with each other in space voids the very idea of an inertial system. This solves, or rather dissolves, Mach's problem regarding the physical grounds for distinguishing inertial systems from non-inertial systems.[15]

Einstein narrates this episode also in an unpublished draft of 1920, where he describes his *Einfall* as "der glücklichste Gedanke meines Lebens" ("the most fortunate thought I've ever had").[16] There he says that he had it while writing the survey paper he submitted to the *Jahrbuch der Radioaktivität und Elektronik* on December 4, 1907, and indeed it is explained in §17 of this paper.

It is clear that if the said two kinds of frame are equivalent, all bodies will experience the same acceleration in a given gravitational field regardless of their respective composition. Moreover, Planck's question concerning the gravitational mass of radiation must then obviously be answered in the affirmative. (For, as Einstein repeatedly pointed out in the sequel, a light ray in an elevator rising with constant acceleration traces out a ballistic trajectory relatively to the elevator's walls.) But, on the other hand, all that this means is that, unless the composition of bodies is irrelevant to gravitational acceleration, and unless light rays are bent by gravity, a frame at rest in a uniform gravitational field *is not* equivalent to a gravitation free frame in uniformly accelerated motion. Thus, Einstein's *Einfall* was not better attested than the exact equality of inertial and gravitational mass, which Planck judged discardable. Indeed, empirical support for it was strictly weaker, insofar as it entailed that radiation would be influenced by gravity in the very way which Planck had declared implausible. Moreover, Einstein was able to show that, if the frames in question were equivalent as stated, natural clocks would slow down as the gravitational potential for their location decreased – a startling forecast for which there was no shred of evidence.[17]

Thus, when Einstein, on the ground of the alleged equivalence of frames, retained the identity of gravitational and inertial mass instead of dismissing it like Planck, he was just betting on a different set of empirically unwarranted predictions. What caused him to prefer them? I can only think of one plausible answer to this question, namely, that Einstein would commit himself to light bending and gravitational redshift rather than to a small discrepancy between inertial and gravitational mass, because those predictions followed from the equivalence of frames, and Einstein would bet on the latter because it signified the end of privileged frames and restricted relativity. This is what I gather from the third paragraph I paraphrased, which I now quote in Schilpp's hazy translation:

> So, if one regards as possible, gravitational fields of arbitrary extension which are not initially restricted by spatial limitations, the concept of the "inertial system" becomes completely empty. The concept, "acceleration relative to space", then loses every meaning and with it the principle of inertia together with the entire paradox of Mach. (Einstein 1949, p. 67.)

To see why this is so, it is advisable to invert, so to speak, Einstein's statement of the equivalence of frames. if, as he says, a frame at rest in a uniform gravitational field is equivalent to a gravitation-free frame uniformly accelerated with respect to the inertial ones, then an inertial frame is, in turn, equivalent to a frame falling freely in a uniform gravitational field. This formulation of Einstein's "equivalence principle" comes in fact closer to his original *Einfall* as he described it in 1920.[18] Indeed, in this version – as Howard Stein has shown – the principle was already stated and profitably employed by Newton, though he was distracted from seeing its full implications by the mirage of absolute space.[19] Einstein, of course, grasped them at once. Since there is no way of escaping gravitation, it is clear that inertial frames can only turn up in the guise of freely falling frames. But a frame of reference may be considered as freely falling in a uniform gravitational field only for a short while – and only if its extension is small. Thus, the equivalence principle, when combined with the pervasiveness of gravity, wrought havoc with the global inertial systems which had replaced Newton's absolute space in modern Galilei invariant and Lorentz invariant physics. Five more years would pass before Einstein lighted on the analogy between curved surfaces and the non-uniform gravitational fields encountered in real life, and was ready to hear Marcel Grossmann's advice about using the Ricci calculus.[20] But the stage was already set for these events. For by dint of the equivalence principle an inertial system could henceforth only be thought of as a local – strictly speaking, tangential – approximation to the world.

Einstein's *Einfall* came out of the blue, but did not therefore stand idle as a gratuitous gift waiting to be validated by blind data. It was certainly a piece of good luck – *der glücklichste Gedanke* – but anyway a thought, and thus a piece and a source of rationality. From the outset it worked as such in the selection and ordering of further thoughts for the planning and reading of observations.

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NOTES

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[1] Cf. C. G. Hempel 1966, p. 16.

[2] "Ein Gedanke kommt wenn »er« will, und nicht wenn »ich« will." (Nietzsche, *Jenseits von Gut und Böse*, §17.)

[3] Norton 1984.

[4] Einstein to Habicht, 24 December 1907. Quoted in Seelig 1957, p. 76, and in Pais 1982, p. 182. I owe the former reference to John Norton.

[5] "Die Gesetze der Schwere und der Bewegungen der Himmelskörper [. . .] haben sich als derart exakt zutreffend erwiesen, daß vom Standpunkte der Erfahrung aus kein entscheidender Grund vorliegt, an der strengen Gültigkeit derselben zu zweifeln." (Einstein 1913, p. 1249.)

[6] In a letter of Sommerfeld of 28 November 1915 – announcing the discovery of the field equations of General Relativity – Einstein noted that the equations of the Einstein-Grossmann theory yielded only a secular advance of 18" instead to the 45" – *sic* – actually observed. (A. Einstein and A. Sommerfeld 1968, pp. 32f.)

[7] "Wenn überhaupt keine Linienablenkung, keine Perihelbewegung und keine Linien-Verschiebung bekannt wäre, wären die Gravitationsgleichungen doch überzeugend, weil sie

das Inertialsystem vermeiden (dies Gespenst, das auf alles wirkt, auf das aber die Dinge nicht zurückwirken). Es ist eigentlich merkwürdig, daß die Menschen meist taub sind gegenüber die stärksten Argumenten, während sie stets dazu neigen, Meßgenauigkeiten zu überschätzen." (A. Einstein, H. and M. Born 1969, p. 258.)

[8] Concerning the empirical adequacy of Newton's theory Poincaré took the same sanguine view that Einstein would express in 1913: "Les observations astronomiques ne semblant pas montrer de dérogation sensible à la loi de Newton, nous choisirons la solution qui s'écarte le moins de cette loi, pour de faibles vitesses des deux corps." (Poincaré 1906, p. 167.)

[9] "Es zeigte sich nun, daß im Rahmen des skizzierten Programmes diesem elementaren Sachverhalte überhaupt nicht oder jedenfalls nicht in natürlicher Weise Genüge geleistet werden konnte. Dies gab mir die Überzeugung, daß im Rahmen der speziellen Relativitätstheorie kein Platz sei für eine befriedigende Theorie der Gravitation." (Einstein 1949, p. 64.)

[10] Planck comments on a "Sachlage, durch welche einige der bisher gewöhnlich als festeste Stütz für theoretische Betrachtungen aller Art benutzten Anschauungen und Sätze ihres allgemeinen Charakters entkleidet werden." (Planck 1907, p. 545.)

[11] "Die Wärmestrahlung in einem vollständig evacuirten, von spiegelnden Wänden begrenzten Raume besitzt sicher träge Masse; aber besitzt sie auch ponderable Masse? Wenn diese Frage zu verneinen ist, was wohl das Nächstliegende sein dürfte, so ist damit offenbar die durch alle bisherige Erfahrungen bestätigte und allegmein angenommen Identität von träger und ponderabler Masse aufgehoben." (Planck 1907, p. 544.)

[12] "Aus sehr präzisen Versuchen (insbesondere aus den Eötvös'schen Drehwage-Versuchen) war mit sehr grosser Präzision empirisch bekannt, daß die *schwere* Masse eines Körpers seiner *trägen* Masse genau gleich sei." (Einstein 1949, p. 64.)

[13] I thank Howard Stein for reminding me that Newton himself resisted the attribution of gravity to matter as an intrinsic property. Commenting on his third *regula philosophandi*, he writes: "Attamen gravitatem corporibus essentialem esse minime affirmo. Per vim insita intelligo solam vim inertiæ." (Newton 1726, p. 389.) He reached the equivalent of eqn. (1) after experimenting with wooden boxes of the same size and shape, suspended from threads of the same length and filled with equal weights of gold, silver, lead, glass, sand, salt, wood, water and wheat. He watched the boxes, diversely filled, go forward and backwards for a long time, with equal vibrations, and concluded that the amount of matter (*copia materiæ*) in the gold was to the amount of matter in the wood as the action of the motive force upon all the gold was to the action of the same upon all the wood, "et sic in cæteris." (Newton 1726, p. 400.) This earthbound result (which, as he noted, was accurate only to 1 part in 1000) he extended to all bodies, everywhere, on the strength of his fourth *regula philosophandi* – "ne argumentum inductionis tollatur per hypotheses."

[14] Cf. Anneliese Maier 1966, Chapter 2; look in particular for Giles of Rome's discussion of a problem in eucharistic physics, as quoted by Maier from Proposition XLIV of his *Theoremata de corpre Christi* (1276). For a short explanation of this matter in English see Torretti 1990, pp. 292–293, note 64.

[15] Einstein 1949, pp. 64f. Mach's problem is stated on p. 62 as follows: "Wie kommt es, daß die Inertialsysteme gegenüber anderen Koordinations-systemen physikalisch ausgezeichnet sind?"

[16] In the unpublished draft version of an invited contribution to *Nature*. See the quotation below, in footnote 18.

[17] Einstein 1907, §19. Einstein proposed a test for it: spectral lines in light issuing from the surface of the sun should occur at a wavelength 1.000002 times larger than those produced by atoms of the same kind on the surface of the earth (Einstein 1907, p. 459). Unfortunately, solar atoms are subject to strong perturbing influences, so that an accurate measurement of the gravitational redshift was to be sooner achieved across a height of 22 m above the ground at MIT, than across the 150,000,000 km between the earth and the sun (cf. Pound and Rebka 1959, 1960; Pound and Snider 1965).

[18] "Then there occurred to me the happiest thought of my life, in the following form. The

gravitational field has only a relative existence in a way similar to the electric field generated by magnetoelectric induction. *Because for an observer falling freely from the roof of a house there exists* – at least in his immediate surroundings – *no gravitational field.* Indeed, if the observer drops some bodies then these remain relative to him in a state of rest or of uniform motion, independent of their particular chemical or physical nature (in this consideration the air resistance is, of course, ignored). The observer therefore has the right to interpret his state as 'at rest'." Translated by A. Pais from the manuscript in the Pierpont Morgan Library; Pais 1982, p. 178.

[19] Newton, *Principia*, Corollary VI to the Laws of Motion, and Bk. I, Prop. III. Cf. Stein's discussion of Mach in Stein 1977, pp. 14–21.

[20] See Norton, 'The chaos of possibilities', pp. 25f. of the draft manuscript.

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